SCIENCE

NEW SERIES
VOL. XLV. No. 1158

FRIDAY, MARCH 9, 1917

SINGLE COPIES, 15 CTS.
ANNUAL SUBSCRIPTION, $5.00

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CONTENTS

MSS. intended for publication and books, etc., intended for review should be sent to Professor J. McKeen Cattell, Garrison-on-Hudson, N. Y.

BOTANY AS A NATIONAL ASSET[1]

IT seems timely to consider the relation of botany to national welfare, when all the sciences are being called upon to render such service as they can in the development of national resources, both material and intellectual. As botanists we realize the important points of contact between our science and human welfare, but the relation between the science of botany and these contacts is not generally appreciated. I am not concerned at present with our usefulness so much as with our reputation, which is in danger of limiting the development of both our science and our service. My attention has been called to this situation in two ways.

1. An increasing number of students of a practical turn of mind are being attracted to the physical sciences because such training is understood to connect definitely with practical activities of various kinds. These sciences are to be congratulated upon having established this connection in such a way that the general public can see it. This has not been accomplished by slighting the fundamentals, but by showing that the fundamentals must underlie all rational practise. Failure to establish this connection in the case of our science means that botany is in danger of being regarded by the general public, and by students who simply record public opinion, as the least practical of the sciences. This attitude is the result of various causes, but chief among them are the attitude of professional botanists, and the fact that the

[1] Address of the president of the Botanical Society of America, New York, December, 1916.

conspicuous practical aspects of botany have been segregated in such special institutions as schools of agriculture, quite distinct from the universities, where by implication only impractical botany is taught.

2. The recent organization of the National Research Council emphasizes the fact that botany must be recognized as a national asset to be developed. The purpose of this council is to bring into cooperation all of those scientific and practical activities which have to do with national welfare. It is an attempt to coordinate the intellectual resources of the country, so that they may be increased and may be available. Since the organization of this council, I have been asked what a botanist has to do with national welfare. This is merely an expression of what seems to be a general feeling, that botany is not a science of human interest, an impression that botanists must correct. This does not mean a revolution in our work, which must deal with the fundamentals, but we must not allow these fundamentals to remain in cold isolation, entirely unrelated to the activities of life. This is not teaching a practise, but developing a vision. In my own experience, I have found that students, while working upon the purely scientific aspects of plants, respond with what seems like gratified surprise to suggestions that all this underlies the possibility of a much more effective handling of plants in supplying human needs.

I wish now to analyze the situation, that we may have it before us clearly; and at the same time to outline the perspective that may change it, and rehabilitate botany in public estimation as the most important of all sciences to human welfare. In fact, I am asking cooperation in arousing the public to a realization of the fact that botany may be made one of the greatest assets of a nation.

We should realize first how the present condition of scholarly isolation has arisen. Men who spend their lives in universities, especially the older ones, are apt to develop certain unfortunate peculiarities. These peculiarities may not make them less happy or less useful to their professional students, but they diminish the appreciation of the community at large. There is a peculiar kind of isolation that is bound to react. It is partly the isolation of a subject which seems more or less remote from general human interests, at least in the aspects the university investigator is cultivating. As a consequence, he feels that his world is quite apart from that one in which the majority of men are living. He is conscious of an interest distinct from their interests, which seem to him therefore relatively trivial. This sense of intellectual aloofness does not result in a feeling of loneliness, but rather in a feeling of superiority; unconscious in many cases, but often naïvely expressed.

It is also the isolation of authority, which comes from the mastery of a subject and association with students who recognize this mastery. To speak with authority in a subject, to give the deciding word, to meet a constant succession of inferiors, is apt to affect any man's outlook on the world of practical affairs. Either he becomes dogmatic in expression, or he must hold himself in check with an effort.

As a consequence, men engaged in fundamental botanical research are apt to be looked upon in general as inoffensive, but rather curious and useless members of the social order. If an investigator touches now and then upon something that the public regards as useful, he is singled out as a glaring exception. If an investigation lends itself to announcement in exceedingly sensational form, as if it were uncovering deep mysteries, the investigator becomes a

marked man, and in all probability he is called a "wizard." The fact is that the great body of investigators, who are doing the substantial work that makes for scientific and practical progress, are unknown to the public. My thesis is that what may be called the pure science of botany should be recognized as underlying all the effective practical handling of plants.

For fear of being misunderstood, I wish to define briefly what I regard as the most important ideal of botany, as of all the sciences. It is to extend the boundaries of knowledge, the goal being to understand nature. This ideal includes no thought of making nature a servant to minister to our needs. To know nature simply because it is wonderful and worth knowing is what it means. Such investigation is like the exploration of an unknown continent. Every advance into the new territory impresses us with the fact that it is far more extensive than we had dreamed. Every trail is worth following because it means additional knowledge. Some trails may lead to rich farm lands and gold mines, but in exploration these are only incidents. To understand the new country, all trails must be followed and mapped.

What may be called practical botany is beginning to realize the importance of exploration. This is indicated perhaps most significantly by the change of attitude in the scientific work of the government. The Bureau of Plant Industry, for example, during the last few years has been adding notably to its staff of scientific explorers. The reason for this has been a realization of the fact that practical application is sterile unless there is a continuous discovery of something to apply.

That scientific exploration is entering upon an advanced stage of its development is shown by the fact that it is proceeding in its method from analysis to synthesis. Until recently progress in botany was marked by an increasing segregation of subjects, so that botanists were distributed into numerous pigeon holes and labeled. A man in one pigeon hole knew little of the work of his colleagues, and cared less. This segregation was immensely useful in the development of the technique of botany; but now we realize the fact that nature is not pigeon-holed, but is a great synthesis; and we know that to understand plants, which is to synthesize our results, all of our so-called sciences must focus upon the problems. We have discovered that to know plants and their relations to the synthesis we call nature, we must know not only their structure and habits, but also the chemistry of the materials that affect their living, the physics of the variable conditions that they must face, the geological record of their changes; in short, botany has become the focusing of all the sciences upon the problems of plants.

In one sense scientific exploration is a luxury, just as music or art or literature, and must be recognized as a response to a high human impulse, the impulse to *know;* but we must correct the impression that botanical exploration is merely a luxury. We have been minimizing our opportunities for botanical research by allowing the impression to continue that our results hold no relation to human welfare.

This impression has been developed chiefly by the fact that two aspects of science are generally recognized, known as "pure" and "applied." There is little general appreciation of the vital connection between these two phases of botany. Not only does the distinction exist in the public mind, but it is reinforced also by published statements from colleges and universities. The distinction seems to be that pure science is of no material service to mankind; and that applied science

ministers to our material needs. The distinction, therefore, is based upon material output. In other words, pure science only *knows* things, while applied science knows how *to do* things. Since the modern American community believes chiefly in doing things, pure science seems to it useless, and the reaction of this sentiment upon opportunities for the cultivation of pure science is obvious.

I must confess that this feeling is too often intensified by those of us who are university investigators. We believe in knowing things, quite apart from their usefulness; and therefore we are in danger of regarding applied science as a waste of investigative energy, and its devotees appear to be unscientific; very useful, but not to be recognized as belonging to the scientific cult, the cult of explorers.

I wish now to outline a campaign of education which should lead to a general appreciation of the fact that botanical exploration can be made our most important national asset. The relation between pure and applied botany can be presented in a series of illustrations by outlining the usual steps that have been taken in the material service of botany to mankind.

In one case an investigator is attracted by a problem. No thought of its usefulness in a material way is in his mind; he wishes simply to make a contribution to knowledge. He succeeds in solving his problem and is satisfied. Later, perhaps many years later, some other scientific man discovers that the results of the former may be used to revolutionize some empirical practise of agriculture. The application is made, but the public hears only of the second man, the one who made the practical application. Obviously, however, both men were of great material service. The ratio that exists between scientific men of the first type and those of the second is not known, but there is very great disparity.

In another case, an investigator is attracted by a problem whose solution may serve the community. He succeeds in solving it, perhaps makes his own application, and is satisfied. Later another scientific man discovers that the results of the former may be used to revolutionize certain fundamental conceptions of biological science. His statement is made and the scientific world recognizes only the second man, the pure scientist, but both men were of large scientific service.

It is evident that responsibility for the practical results of our science is to be shared by those engaged in pure science and those engaged in applied science. The only distinction, therefore, is not in the *result,* but in the *intent.* In fact, the difference between pure science and applied science in their practical aspects resolves itself into the difference between murder and manslaughter; it lies in the intention. In every end result of science that reaches the public there is an inextricable tangle of contributions. Between the source of energy and the point of application there may be much machinery, and perhaps none of it can be eliminated from the final estimate of values. And yet the public has been gazing at the practical electric light, and forgetting the unseen and therefore apparently impractical power house.

All science is one. Pure science is often immensely practical, applied science is often very pure science, and between the two there is no dividing line. They are like the end members of a long and intergrading series; very distinct in their isolated and extreme expression, but completely connected. If distinction must be expressed in terms where no sharp distinction exists, it may be expressed by the terms "fundamental" and "superficial." They are terms of comparison and admit of every intergrade. In general, a university devoted to research should be interested in

the fundamental things, the larger truths that increase the general perspective of knowledge, and may underlie the possibilities of material progress in many directions. On the other hand, the immediate material needs of the community are to be met by the superficial things of science, the external touch of the more fundamental things. The series may move in either direction, but its end members must always hold the same relative positions. The first stimulus may be our need, and a superficial science meets it, but in so doing it may put us on the trail that leads to the fundamental things of science. On the other hand, the fundamentals may be gripped first, and only later find some superficial expression. The series is often attacked first in some intermediate region, and probably most of the research in pure science may be so placed; that is, it is relatively fundamental, but it is also relatively superficial. The real progress of science is away from the superficial toward the fundamental; and the more fundamental are the results, the more extensive may be their superficial expression.

In our campaign of education, which is to develop some appreciation of the fact that botanical exploration is a great national asset, concrete illustrations must be used to show that what people regard as applied science, which seems to them therefore useful and worthy of support, is but a superficial expression of fundamental things which it is the mission of pure science to discover. In other words, it must be known that the most practical science in the long run is the most fundamental.

I wish to illustrate my meaning by one concrete example, selected from many that will occur to any botanist. This will indicate how we can make the contacts between our pure science and the human welfare appreciated. The science of botany has had an interesting history. Beginning with the investigation of plants for what were called their "medicinal virtues," it developed with various progressions and retrogressions, until the botanist came to be regarded as about the most useless intelligent member of society. His chief concern seemed to remove him so far from the general human interest that he was regarded as a harmless crank at best, a man of only ephemeral interest. The most unfortunate result was that this public estimation of botany lingered much longer than it was deserved; and consequently, when the other sciences had won public esteem, either through their services or their appeal to the wonder instinct, botany lagged behind in public recognition, and in most educational institutions was the latest born in the family of sciences; but finally it also began to render signal service and appeal to the wonder instinct.

Among the several phases of botanical activity, phases which deal with the fundamentals of plant activity of all kinds, and are directly related to plant production, I wish to select plant breeding as a single illustration. It is not my purpose to recite the notable achievements that can be grouped under this title, for they are familiar to all of you. I wish simply to use plant breeding as a brief and concrete illustration of my thesis.

The *practical* aspect of plant breeding in a certain sense is as old as the cultivation of plants. Long experience in the practical handling of plants developed a kind of knowledge that became formulated in empirical practise; that is, practise whose meaning was not understood, but whose result experience assured. In general, the improvement of old forms by continuous selection grew into a fairly successful empirical practise.

During all this period of plant improve-

ment by selection, the so-called science of botany was cultivating a singularly distant field. In short, botany was not practical, and plant breeding was not scientific. As a consequence, botanists, on the one hand, and agriculturists, horticulturists, etc., on the other hand, were as distinct from one another as if they had nothing in common. It so happened that botanists were dealing with superficial problems in a scientific way, and plant breeders were dealing with the most fundamental problems in an empirical way.

As in any practise, plant breeding developed now and then an unusually successful practitioner, who made distinct contributions in the form of important results; but this represented no more of a real advance than does the fact that one cook can surpass another cook in the art of making bread.

What may be called the second period of plant breeding was ushered in when organic evolution began to be put upon an experimental basis. Plant breeding had been practical, but with no scientific basis; now a new plant breeding was established which was scientific, but with no practical motive. The new motive was the accumulation of data bearing upon the problem of inheritance. As a by-product of this work on inheritance, some of the scientific results have been applied to practical plant breeding, and the result has been an expansion of its possibilities that may well be called marvelous. In short, practical plant breeding is now on a scientific basis, and botany has at last attacked the fundamental problems and is beginning to be of great practical service.

In presenting this fleeting glimpse of the problems and accomplishments of plant breeding, I have attempted to emphasize the inextricable entanglement of pure and applied science. Any result of scientific plant breeding, representing as it must additional knowledge of the processes of inheritance, may become of practical service; and any result of practical plant breeding, involving as it does extensive experiments with plants, may prove to be of great scientific value. They are mutually stimulating, and both are necessary to the most rapid development of knowledge. This suggests that the botanical perspective to be developed in our campaign of education might be stated as practise based on science, and science that illuminates and extends practise.

In connection with the organization of the National Research Council, I feel that American botany is offered a great opportunity of which we should take advantage. As a member of the council I wish to acquaint you with its purpose, so far as botany is concerned. Since the organization of the council was stimulated by the desire to develop a program of national preparedness, the natural first impression would be that, so far as botany is concerned, it is merely the problem of more efficient food production and distribution. This would stamp the enterprise at once as a problem of practical agriculture, in connection with which botanical investigators who are dealing with the fundamental problems of plants would have little or no part. Nothing is further from the intention of the council. The chairman has recently outlined the work of the council briefly as follows:

1. To prepare a national census of research, showing what laboratories and investigators are available.

2. To encourage the cooperation of educational and research institutions in working out problems of pure science and industry.

3. To promote research in various branches of science in cooperation with leading national scientific societies.

4. To encourage scientific research in educational institutions. It is proposed, for instance,

that in each advanced educational institution there be a committee on research to promote original investigations on the part of the faculty and graduate students.

5. To establish research fellowships in educational institutions, thus affording qualified workers an opportunity to devote themselves entirely to research work.

6. To secure wherever possible endowments for research purposes.

It is evident that so far from being primarily work in the practical application of what we know already, the enterprise is intended to be primarily a stimulus to fundamental research in every direction. It is not *practical application* that is to be stimulated chiefly, but *exploration*, which may or may not result in practical application. It is felt, for example, that the more we know about the structures and activities of plants, the better equipped we shall be to handle plants intelligently. Our botanical program, therefore, is simply to extend the boundaries of our knowledge of plants as far as possible. In pursuance of this program, at least two things are felt to be necessary.

In the first place, there must be developed some scheme of cooperation among our botanical establishments; and notably between the research establishments and the so-called practical establishments. For example, we recognize in general three great botanical agencies at work to-day, working independently, and in too great ignorance of each other's results. These agencies are the Department of Agriculture, the agricultural colleges and experiment stations, and the universities. All of these agencies are investigating plants from various points of view, but they are not as mutually helpful, or even as mutually stimulating as they should be in the interest of progress. I have met many cases of men intellectually equipped to work, but with no adequate material or equipment; and also even more cases of fine equipment and abundant material, and no man trained to use them effectively. In other words, the distribution of men and equipment is not as effective as it should be.

In the second place, there must be developed some plan of supporting research wherever there is a competent investigator. The movement to establish research fellowships has begun already, and as the value of research becomes better understood, there is no reason to doubt that every botanical explorer will have the opportunity to explore. There is at present a tremendous amount of waste in the investigators produced by the universities. Every year scores of young investigators, well equipped to continue exploration, are automatically side-tracked by a degree, and forced into positions where investigation is killed, or at least becomes anemic. The council proposes to conserve some of this investigative ability, and to give it a chance to express itself. In short, the opportunity now presented to us is to increase the opportunities for botanical research to such an extent by cooperation and conservation of investigative ability that the progress of botany should take on a greatly increased momentum. And all this can be done if at this psychological moment we as botanists can make it clear that a fundamental knowledge of plants is a great national asset.

JOHN M. COULTER
UNIVERSITY OF CHICAGO

THE COMMITTEE OF ONE HUNDRED ON SCIENTIFIC RESEARCH OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

REPORT OF THE SUBCOMMITTEE ON ENGINEERING

IN view of the fact that a subcommittee on engineering has only very recently been appointed by the American Association for the Advancement of Science committee of one

hundred on scientific research, no report concerning actions or accomplishment can be submitted at this time. A few suggestions may be offered, however, bearing upon the activities of an engineering research committee.

Researches in engineering may be divided into two classes; namely (1) those undertaken for the assistance of some particular industry or manufacture, and (2) those undertaken for the advancement of technical knowledge or applied science. Industrial researches are numerous, and have a great history; but very little literature. They also have a great range of character and intensiveness, say from overcoming, in a few minutes, certain little mechanical difficulties in the behavior of a machine, to scientific investigations pursued systematically for years, perhaps with a staff of trained technicians, say for the purpose of developing some important industrial process. The amount of industrial research going on in the country is, in the aggregate, very great, and is likely to increase as time goes on and industries further specialize. Many of these problems of industrial research fall within the professional province of consulting engineering, and indeed few new large engineering undertakings can be met without involving some new industrial research. The characteristic quality of industrial research is that it does not find direct or immediate publication. Probably much of it is eventually published either in the form of engineering data or in patent specifications; but the competition between branches of industry almost inevitably demands that the scientific or technical underlying progress should be protected. The industrial value of successful researches may be so great that their nondisclosure is a first consideration in reward of the necessary labor and expense. The hope and justification of industrial research is that it may, and often does, pay for itself. In many instances it has paid most handsomely.

On the other hand, the second type of research, *i. e.*, engineering researches or researches for engineering development may also cover a great range of quality and intensiveness from say brief tests of the performance of some machine, to elaborate investigations in mathematics, physics, chemistry or economics. Such engineering researches may either be intensely practical on the one hand; or they may be outside of the immediate fields of scientific application, and differ in no evident way from so-called pure scientific enquiry, paving and prospecting the ways for future use in engineering. The characteristic quality of these engineering researches is that in-so-far as they are successful they tend to find direct and immediate publication, and so to become available for the use of all concerned.

It is evident that engineering technological researches may be of great value to a country or to industries; but that they inherently lack self-support. Any laboratory engaged in researches, the successful results of which are to be published, can only expect to be supported either by national institutions, by gifts, or by benevolent endowment. For this reason, although industrial researches are numerous and widespread, engineering researches are mainly restricted to universities, technical colleges and government laboratories.

It would seem to be desirable that the laboratories in which engineering researches are carried on should all be brought into some cooperative association, not only for mutual benefit, but also for the benefit of engineering, and of the country at large, through engineering. It is desirable that each and every technological laboratory should develop a specialty or a group of specialties. The tendency in the past has been for essentially the same course of engineering studies to be pursued in the technical colleges, and in natural conformity therewith, the laboratory investigations have been more or less of the same type. In order, therefore, to develop greater advantages from cooperative effort, such specialties as happen to develop in the researches of technological laboratories should be fostered and encouraged, so long as freedom of individual action is not restricted.

The encouragement of specializing in engineering researches, for the greater good of

the country and of the world, can be effected by the Association for the Advancement of Science through the recognition of such specialization as spontaneously occurs, through grants for the solution of particular problems, and through assistance in finding adequate publication for the results that may be obtained from engineering researches.

A. E. KENNELLY

CAMBRIDGE, MASS.

REPORT OF THE SUBCOMMITTEE ON PATHOLOGY

THE Committee on Research in Pathology recommends:

1. *Nature of Work to Be Aided.*—In the awarding of grants that preference be given to problems of etiology, immunity, functional pathology and chemical pathology, as representing the most profitable lines of investigation at present.

2. *Laboratories or Individuals to Be Aided.*—It is believed advisable to give grants preferably to laboratories presided over by a director of known training and ability in investigation, the funds of which are insufficient to meet the needs for special studies. This does not necessarily rule out an exceptional man in a laboratory indifferently manned, but it must be remembered, as a general proposition, that laboratories which need the money most are, on account of poor equipment and the lack of adequate staff, least prepared to use it to advantage. The best policy is to give where most can be accomplished and not where money is most needed.

It is undesirable to give money solely to encourage research in a general way by younger men under direction of the laboratory head. The aid should be for a definite problem of recognized importance and should be preferably to men of wide experience as investigators, and as far as possible to heads of departments, who will take an active part in the work, aided perhaps by their assistants.

In addition to departments of pathology, those of bacteriology, protozoology and immunology, or clinical medicine possessing well-equipped laboratories for investigation along any of the lines before mentioned shall be considered as conducting research in pathology and eligible for grants. The sole conditions for the award of a grant should be (1) The formulation of a suitable problem; (2) the proposal of definite methods for its solution; (3) the possession of facilities adequate for the successful prosecution of the projected investigation.

3. *Amount of Grants.*—In view of the position taken in Section 2 it is recommended that grants of relatively large sums (several hundred dollars) be given to a few laboratories rather than smaller sums scattered more widely. These larger sums would ensure, presumably, an adequate return and would offer a greater incentive to concentrated work on important problems.

4. *Cooperation.*—It is considered desirable for the committee to keep in touch with other organizations, as the Rockefeller Institute for Medical Research and the Research Committee of the American Medical Association, offering grants for research in medicine in order (*a*) to avoid duplication of grants, (*b*) to exchange lists of applicants, (*c*) to profit by the experience of these organizations.

5. *Publicity.*—It is considered inadvisable to issue a general request for applications. The publication in SCIENCE and the *Journal of the American Medical Association* of the report of the committee should place the matter before the proper audience and lead to requests from individuals, presumably heads of departments most interested in such aid and best prepared to take advantage of it.

THEODORE C. JANEWAY,
EUGENE L. OPIE,
H. GIDEON WELLS
RICHARD M. PEARCE (chairman),
University of Pennsylvania,
Philadelphia, Pa.
PEYTON ROUS (secretary),
Rockefeller Institute,
New York City

REPORT OF THE SUBCOMMITTEE ON MATHEMATICS

IN view of the proposed plan to form research committees with the direct cooperation of various scientific organizations, the

subcommittee on mathematics decided not to attempt to present a formal report at this time. I desire, however, to take advantage of this opportunity to emphasize the fact that mathematical research is probably no less dependent upon financial support than research in the other scientific subjects.

It is true that the mathematical investigator seldom needs costly equipment beyond good library facilities, but what he gains in this direction he loses as a result of the fact that his most important discoveries frequently require very extensive development before they are fully appreciated even by the mathematical public. In some scientific fields discoveries of the greatest popular interest can be announced effectively in a few words, and hence the publications necessary to meet the direct needs of the investigator in these fields are comparatively inexpensive.

The lack of funds for the publication of extensive mathematical treatises and memoirs has had very baneful consequences. In the case of treatises on modern subjects the scientific value often increases much more rapidly than the size of the treatise. If an author who is perfectly competent to prepare a treatise of six hundred pages on such a subject is compelled to limit himself to four hundred pages, he usually finds it necessary to omit the developments which are most original and which would reflect most honor on the author and on the country in which the work is published.

The intrinsic scientific value of mathematical memoirs is usually not very seriously affected by brevity in presentation. On the other hand, this brevity tends to reduce the immediate influence of these memoirs, since it increases enormously the difficulties met by those who try to master them. The mathematical reader is often compelled to waste much time in trying to decipher what the author could have exhibited clearly if he had had a few more pages at his command. As compared with European publications American mathematical literature includes a comparatively small number of extensive memoirs.

The most expensive element tending to improve research conditions is the providing of sufficient free time for the investigator. In this respect mathematics does not present a problem which differs materially from that presented by other subjects, unless it is assumed that the very abstract nature of his subject makes it unusually difficult for the mathematician to utilize odd moments. At any rate, I hope I have succeeded in making clear that American mathematical research could be greatly improved by more liberal financial support, and I presume the importance of mathematical developments needs no emphasis before a body of scientists.

G. A. Miller,
Chairman

SCIENTIFIC NOTES AND NEWS

Dr. J. H. Long, dean of the school of pharmacy and professor of chemistry at Northwestern University, has been elected president of the Chicago Institute of Medicine for the year 1917.

The Royal Geographical Society of London has elected Dr. Charles Doolittle Walcott, secretary of the Smithsonian Institution, a corresponding member.

Dr. J. J. R. Macleod, professor of physiology, school of medicine, Western Reserve University, has been granted leave of absence to act as professor of physiology in McGill University, Montreal, during the months of February and March.

Professor Waldemar Lindgren, of the Massachusetts Institute of Technology, has gone to Chile in connection with geological work on some of the copper properties.

The honor of knighthood has been conferred on Professor Jagadish Chandra Bose, of Calcutta, known for his work in physics and physiology.

Professor A. N. Whitehead has been elected president of the British Mathematical Society.

Major P. A. MacMahon has been elected president of the Royal Astronomical Society in succession to Dr. R. A. Sampson.

Dr. Smith Ely Jelliffe, of New York City, has been appointed editor of the *New York*

Medical Journal to fill the vacancy caused by the death of Dr. Claude L. Wheeler. Dr. Jelliffe was editor for several years of the *Medical News* before that periodical was discontinued, and is now managing editor of the *Journal of Nervous and Mental Disease.*

DR. MARY GAGE DAY, a sister of Professor S. H. Gage, of Cornell University, is leaving Kingston, N. Y., to make her home with her brother in Ithaca and devote her time to researches in biology. During the last twenty years she has practised medicine in Kingston.

MR. F. E. KEMPTON, assistant in the department of botany at the University of Illinois, has been granted leave of absence to take up the work of a plant disease survey for the St. Louis Smelting and Refining Company in the vicinity of their plant at Collinsville, Ill.

THE Arkansas Academy of Sciences was formally organized on January 11 at a banquet held at Little Rock for that purpose. Officers for the ensuing year are Charles Brookover, president; Morgan Smith, vice-president; Dewell Gann, Jr., secretary; Herbert A. Heagney, treasurer; Troy W. Lewis, permanent secretary. The meeting for 1917 will be held at Little Rock on October 12 and 13. We are requested to state that the Arkansas Academy of Sciences desires to affiliate with other scientific societies.

THE following awards of the Society of Engineers (incorporated) were presented on February 5: The president's gold medal to Professor C. G. Cullis for his paper on "The Mineral Resources of the British Empire as regards the Production of Non-Ferrous Industrial Metals"; the Bessemer Premium to Professor W. G. Fearnsides for his paper on "The Mineral Requirements of the British Iron and Steel Industries"; the Bernays Premium to Professor J. A. Fleming for his paper on "Engineering and Scientific Research"; the Nursey Premium to Mr. J. E. Lister for his paper on "Modern Coal and Coke Handling Machinery as used in the Manufacture of Gas"; and the Society's Premium to Mr. Ewart S. Andrews for his paper on "The Design of Continuous Beams."

A COMMITTEE of the Cornell Society of Civil Engineers is receiving contributions to a fund for a testimonial to Professor Irving Porter Church. A part of the fund will be expended for a portrait of Professor Church to be presented to the university. The remainder will be used for a gift to the university in his honor.

AN oil portrait of Professor O. T. Bloch, according to the *Journal* of the American Medical Association, was hung recently with appropriate ceremonies in the hall of the Surgical Academy at Copenhagen, in preparation for his approaching seventieth birthday. The surplus left from the subscriptions for the portrait was presented to him, and he turned it over to the building fund of the medical society. He was for a long time on the editorial staff of the *Hospitalstidende* and has published numerous works in this and in Scandinavian, British and French surgical journals. He has also published several books, including one on the history of treatment of wounds from the earliest to modern times.

DR. MARTIN H. FISCHER and Joseph Eichberg, professors of physiology, University of Cincinnati, and Dr. Ludvig Hektoen, head of the department of pathology, University of Chicago, have been elected Cutter lecturers on preventive medicine and hygiene at Harvard University for the academic year 1916–17.

DR. A. HOYT TAYLOR, head of the department of physics of the University of North Dakota, has given a series of two lectures at Northwestern University and the Chicago Academy of Science on "Recent Advances in Radio Communication" with demonstrations of wireless telephony and telegraphy. Radio signals were amplified so as to be audible to an audience of several hundred and a musical concert was received by wireless telephone from a station some eight miles distant.

MR. JONATHAN HUTCHINSON gave the Hunterian lecture before the Royal College of Surgeons of England on February 12 on "Dupuytren's Life and Surgical Works."

ARNOLD VALENTINE STUBENRAUCH, professor of pomology in the University of California,

died at his home in Berkeley on February 12, 1917. A graduate of the University of California of 1899, Professor Stubenrauch was for ten years in the U. S. Department of Agriculture, resigning in 1914 his position as pomologist in charge of field investigations to return to service in the University of California. He was the first man to demonstrate that dates could be grown with commercial success in the Imperial Valley, on the desert in southern California; in association with G. Harold Powell he developed the pre-cooling method, which has greatly contributed to success in the shipping of fruit from California; he demonstrated that California grapes could be kept safely in cold storage for months if packed in redwood sawdust. He was of unusual power as a teacher and a stimulator of scientific activity.

Dr. C. V. Burton, known for his contributions to experimental and theoretical physics, died on February 3, owing to an accident at a British aircraft factory.

The death in Paris is announced of Dr. Jules Dejerine, a member of the Paris Academy of Medicine and well known as a neurologist, at the age of sixty-eight years.

A. Pappenheim, privat-docent at the University of Berlin, noted for his work on the morphology of the blood and the blood diseases, editor of the *Folia hæmatologica,* and the author of a work on the chemistry of dyestuffs, recently succumbed to typhus acquired in his professional work.

It is stated in the *Experiment Station Record* that plans have been approved by the building committee for the new agricultural building at the Maryland College for which $175,000 was appropriated by the last legislature. A three-story and basement structure, with a front wing 200 by 68 feet, connected by an enclosed bridge with an auditorium seating about 1,000 people, and this in turn connected with a rear wing of the same dimensions as the front, is contemplated. The front wing is to be used for offices and classrooms and the rear wing for stock judging and exhibitions and experimental work. It is hoped that the structure will be ready for use next fall.

According to the *Journal* of the American Medical Association, Dr. Raymond Tripier, of the School of Medicine, Lyons, whose death was announced in December, bequeathed to the University of Lyons 200,000 francs for the encouragement of special work in operative medicine and pathologic anatomy. He also bequeathed to the city of Lyons the sum of 200,000 francs, the annual interest of which will permit the acquisition of a work of art every five years.

The Liebig Scholarship Society of Germany has recently been formed, with a capital of upwards of a million marks from German industries, for the purpose of assisting young German chemistry students to proceed with their studies, after their examinations, by working as assistants in the technical high schools.

There has been organized at the University of North Carolina a mathematical club whose members are drawn from the instructors and graduate students of the mathematical and allied departments. The following officers have been elected: Wm. Cain, president; Archibald Henderson, vice-president; J. W. Larley, Jr., secretary.

The third annual meeting of Entomological Workers of Ohio was held at Ohio State University on February 2, with thirty members in attendance. The program consisted of reviews of projects and reports on investigations of members of the Ohio Experiment Station, the State Division of Orchard and Nursery Inspection and the department of entomology of the university.

The council of the British Association of Chambers of Commerce is, as we learn from foreign exchanges, considering draft bills designed to carry out reforms in our systems of weights and measures and of coinage, and should the council approve of them they will be submitted to the Chambers of Commerce throughout the country. If there proves to be general agreement the association's bill will be introduced into Parliament. It is probable that a bill for establishing a decimal coinage will have first attention, the bill for introducing metric weights and measures not being pressed until the country has grown accus-

tomed to a decimal coinage. It is suggested that the simplest means of making the change would be the adoption of the present florin, which is the tenth part of a sovereign, as the unit. The existing farthing would be replaced by a "cent," equal to the hundredth part of a florin, instead of a ninety-sixth part as now. Sir Edward Holden, at the meeting on January 26 of the London City and Midland Bank, of which he is chairman, expressed himself strongly in favor of the adoption by Great Britain of the metric system.

A PRESS bulletin of the Geological Survey calls attention to the fact that the press dispatches describing the latest eruptions of Lassen Peak show a continued tendency to refer to the volcano as Mount Lassen. Perhaps it is thought that the name should correspond with those of some other famous peaks of the Cascade Range, such as Mount Shasta or Mount Rainier. But Lassen Peak, as the most active and interesting volcano in the United States, is especially entitled to be called by its own name, and acts of Congress and Presidential proclamations in creating and recognizing the Lassen Peak National Forest and Lassen Peak National Monument have given the name Lassen Peak a status of high rank in the geologic annals of the Cascade Range. The area has recently been set apart as the Lassen Volcanic National Park. The name Lassen Peak, according to the United States Geological Survey, Department of the Interior, is the only authorized form on maps, reports and gazetteers from the Whitney Geological Survey of California, in 1865, to the geomorphic map of California and Nevada published by the Earthquake Investigation Commission, as well as on the latest map issued by the Forest Service. Peter Lassen, the sturdy pioneer who guided many an early settler to the sunny lands of the Sacramento, lies buried in a lonely grave in Lassen County. A small, crumbling monument thirty miles from the peak marks his final resting place, but his greater and more enduring monuments are the county and peak named in his honor by a grateful people. The snow-capped Lassen Peak has piloted many an immigrant to the mountain pass. In the early days of the Pacific Railroad surveys some pious monk called the peak St. Joseph's Mountain, but the names Lassen's Peak and Lassen's Butte soon came into general use. Whitney has shown the inappropriateness of the French term butte, which, translated exactly, means knoll. As Lassen never owned the mountain, in later years the possessive form of the name was dropped, and to correct an illicit tendency to wander from well-established usage the United States Geographic Board, in its decision of October 9, 1915, officially recognized the fact that the name of the mountain was *Lassen Peak,* not Mount Lassen.

THE United States Civil Service Commission announces an examination for expert electrical and mechanical aid, to fill a vacancy in this position at $12.48 per diem, in the Bureau of Yards and Docks, Navy Department, Washington, D. C., and vacancies as they may occur in positions requiring similar qualifications. The duties of this position cover the expert maintenance and supervision of the operation of all navy-yard power plants, embracing the economical production, distribution and utilization of electric power for manufacturing, pumping dry docks, charging submarines, and tral heating, and production and distribution of for manufacturing; steam for power and central heating, and production and distribution of hydraulic power; also investigations of power-plant operating conditions, tests of plants and equipment, and efficiency engineering work in connection with improvement of operating conditions and instruction of plant operatives to obtain economical operating results. Competitors will not be assembled for examination, but will be rated on technical education, experience and fitness. Graduation with a degree of mechanical engineer or electrical engineer from a college or university of recognized standing, and at least ten years' subsequent experience in responsible charge of the design, installation and operation of central power plants and distribution systems for light, heat and power, with executive experience in handling successfully large numbers of power-plant employees, are prerequisites for consideration for this position.

A LETTER received at the Harvard College Observatory from Professor Henry Norris Russell, director of the Princeton University Observatory, contains the following preliminary values of the parallax of the star of large proper motion in Ophiuchus which have been determined by him from micrometric observations communicated by Professor Barnard. From the differences of the distances of stars a and k, a solution in which the proper motion is eliminated in the usual manner gives a parallax of $0''.69 \pm 0''.06$. Measures of positives made from the plates of 1894 and 1904, when compared with the measures of 1916, give a proper motion of $10''.38$ toward 355°.8. Assuming this proper motion, the distance measures of the stars a, c and k give parallaxes of $0''.85$, $0''.53$ and $0''.66$, and the measures of position angle a mean parallax of $0''.75$. The mean of these determinations is $0''.70 \pm 0''.05$. The absolute magnitude of this star on Kapteyn's scale is 13.6, and its real brightness is less than one three-thousandth that of the sun, making it the faintest star so far known.

UNIVERSITY AND EDUCATIONAL NEWS

THE University of California has received through the death of Mrs. Elizabeth Josselyn Boalt an endowment of $200,000 for the maintenance of professorships in law.

AT a meeting of the governors of the South Wales University College, called to consider proposals for providing better science teaching after the war, it was reported that Sir W. J. Tatem had promised to provide a chemical laboratory which would cost at least £25,000, and that other promises included 1,000 guineas from the late Mr. Beaumont Thomas, 1,000 guineas from Mr. Dan Radcliffe, 1,000 guineas from Mr. J. Herbert Cory, M.P., 1,000 guineas from Mr. W. Beyron, and 2,000 guineas each from Mr. Morgan Wakely and Mr. Percy Miles.

HAVING decided to open its courses to women as soon as proper facilities can be provided, the college of physicians and surgeons, Columbia University, is now appealing for immediate contributions of $50,000, so that these facilities may be secured and women admitted to the school next September. It is proposed to erect a small addition to the present college buildings, sufficient to provide for the women students, until such time as new quarters are provided for the entire college.

ON February 23 the regents of the University of Michigan adopted a resolution confirming the union with the University of Detroit college of medicine and surgery. The terms of the merger are that the latter shall turn over its charter, real estate, equipment and hospital privileges to the university and that a fund of a million dollars will be raised for the development by the university of a graduate school of medicine in Detroit.

DR. THEODORE LYMAN and Dr. George W. Pierce have been promoted to professorships of physics at Harvard University.

DR. GEORGE B. PEGRAM, professor of physics at Columbia University, has been appointed to be dean of the school of applied science to succeed Frederick A. Goetze, who is now treasurer of the university.

PROFESSOR RAYMOND C. OSBURN, professor of biology at the Connecticut College, New London, Connecticut, has been elected head of the department of zoology and entomology of the Ohio State University, his appointment to take effect July 1. He will assume the duties carried for the last nineteen years by Professor Herbert Osborn, who was last year elected research professor and who will hereafter give his entire time to research work, including the direction of research by graduate students, and, for the present, the directorship of the Lake Laboratory and of the Ohio Biological Survey.

PROFESSOR DR. O. VAN DER STRICHT, professor of histology and embryology, University of Ghent, Belgium, has been reappointed research fellow in cytology, school of medicine, Western Reserve University.

DR. P. N. VAN KAMPEN, university lecturer at Amsterdam, has been appointed professor of zoology and comparative anatomy in the University of Leyden, in succession to the late Professor Vosmaer.

DISCUSSION AND CORRESPONDENCE

A CULTURE MEDIUM FOR EUGLENA

A MEDIUM discovered quite by accident has enabled the writer to carry on vigorous cultures of *Euglena* for a period of more than a year. Some five hundred cultures have proved conclusively that it is a success. The medium is quince-seed jelly, which is in common use as an agent for retarding the movements of Protozoa. It is prepared by boiling quince seed in distilled water, passing the thick, glutinous mass which is obtained through a sieve to remove particles of the seed and then diluting with distilled water to the desired consistency. Cultures have been carried in test tubes, jars, flasks and other receptacles. Some tubes remained corked throughout the entire year and were found to contain virile cultures at the end of that time.

The jelly seems to be specific for *Euglena*, some other chlorophyll-bearing Protozoans and for bacteria. Tubes were inoculated with cultures of mixed Protozoans and after a period of two months only the *Euglena* and a minute green flagellate survived, the other Protozoans living only as long as the supply of bacteria lasted.

Two hundred successful transplants have been made from a single culture.

The medium has several obvious advantages:

1. It enables the operator to carry on cultures for a long period of time without giving them constant attention.

2. The medium is viscid and evaporates rather slowly.

3. A constant as regards density and chemical content may be obtained for experimental work by evaporating the medium to dryness and making up a standard solution with distilled water.

The results of a year's experiments together with some notes on the behavior of *Euglena* are soon to be published.

CLARENCE L. TURNER
DEPARTMENT OF ANATOMY AND BIOLOGY,
MARQUETTE UNIVERSITY SCHOOL OF MEDICINE

A RELIEF MAP OF THE UNITED STATES

TO THE EDITOR OF SCIENCE: The article entitled "Expedite the Map," which appeared in the October 13 issue of SCIENCE, brings to mind the desirability of having in the city of Washington, suitably housed, a large scale relief map or model of the United States.

This model might be about 300 feet square or 600 feet square, according to the structural difficulties which would be encountered and the amount of appropriation which could be obtained from Congress or other source.

As to the appropriation, I doubt that it would be easy to secure funds from Congress for an object of this kind, and I believe that it would be better to depend upon private philanthropy to secure the financial foundation needed.

The statement "Every industry, art and science which demands a knowledge of the lay of the land is benefited by good maps of the area in which it is carried on," and the remainder of the paragraph from which this sentence is quoted apply equally to a relief map.

Such a map, if constructed, would be available for consultation by members of Congress, bureau officials and by the general public; and it would be one of the sights of the national capital. The main problem is to find the philanthropist.

T. W. KINKAID

LEIDY ON THE CAUSE OF MALARIA: A CORRECTION

IN a letter to Professor Henry Fairfield Osborn, published by him in his "Biographical Memoir of Joseph Leidy,"[1] I stated that in 1853 Leidy "discussed the cause of malaria and wrongly concluded that it is not of parasitic origin." Dr. Joseph Leidy, 2d, has kindly called my attention to my regrettable blunder. What Leidy really said[2] was:

> That malarial and epidemic fevers have their origin in cryptogamic vegetables or spores requires yet a single proof. If such were the case, these minute vegetables and spores, conveyed through the air, and introduced into the body in respiration, could be detected.

FRANK E. LUTZ

[1] National Acad. Sci., Biographical Memoirs, VII., 1913, p. 356.

[2] Smithsonian Contrib. Knowledge, V., 1853, p. 14.

SCIENTIFIC BOOKS

Water Supply. By William P. Mason. Fourth Edition. New York, John Wiley and Sons. x + 528 pages, 6/9. $3.75 net.

The fourth edition of Professor Mason's well-known book on water supply testifies to the high esteem in which this book is held by the American engineering public. Published originally in 1896 it has passed through subsequent editions, each time being substantially enlarged and improved. For the present edition a large amount of the text has been entirely rewritten and suitable amount of new material added. The tables have been brought up to date and new photographs introduced. Some of the most noticeable changes are the following:

The chapter on Drinking Water and Disease has been strengthened by the addition of many pages devoted to typhoid fever. The work of recent years is drawn upon to set forth present-day conceptions in regard to the existence of the typhoid bacillus outside its human host and in "carriers." The distribution of the disease and factors operating in its transmission are also discussed. Considerable material has been withdrawn from this edition relative to the now discredited theory of water-borne malaria.

Newly developed methods of water purification, particularly processes aiming at disinfection, come in for consideration, as do certain newly found factors influencing natural purification in streams and stored waters. The use of chlorine ozone, ultra-violet light and copper sulphate receive attention. There is considerable discussion of various phases of the pollution of drinking water supplies and the care of watersheds.

Revisions and additions appear frequently throughout the chapters dealing with ground water and with the corrosive action of water. The appendices deal with entirely new subjects and are brief.

Professor Mason is always a pleasing writer and has the art of abstracting the important data from the writings of others and presenting them in an attractive form. Although this can not be called an exhaustive treatment of the subject it is one of the most interesting and suggestive treatises on water supplies published since the old book of the same title by Professor William Ripley Nichols, of the Massachusetts Institute of Technology.

George C. Whipple
Harvard University

Sarcophaga and Allies in North America. By J. M. Aldrich. Published by the Entomological Society of America. Lafayette, Indiana, 1916.

In 1915 the Entomological Society of America, recognizing the difficulty of adequately publishing monographs on American insects, established the Thomas Say Foundation for this purpose. Subscriptions were solicited, and the accumulating funds were set aside to be used from time to time as suitable works might be offered for publication. The plan is not unlike that of the English Ray Society, which has been publishing important zoological works for many years. Very appropriately, the Foundation is named after Thomas Say, the founder of American entomology. The first monograph issued under these auspices is now before us, and is a revision of the Sarcophagid flies, commonly known as flesh-flies, by Dr. J. M. Aldrich. These flies, which are very abundant in America and Europe, and in some cases of considerable economic importance, have long been the despair of students. It was recognized that the species were numerous, and in fact over a hundred supposed species had been described, but no one could satisfactorily identify them. About twenty years ago L. Pandellé published a work in France, in which he separated the European species known to him by the characters of the sexual organs. This method proved brilliantly successful, and after a time was confirmed and adopted by the other European workers. It is now applied to the American flies, with the result of making the whole subject over, and replacing chaos by order. Dr. Aldrich has been able to recognize 145 species and varieties in the American fauna, and figures the genitalia of 138. Every reasonable effort has been made to identify the earlier

described forms, but since the older authors had little or no conception of the true specific differences in this group, many names have necessarily been set aside as practically meaningless. The treatment throughout is full and sufficient; the genera and species are separated by keys, and the descriptions of the species are quite detailed. Types are carefully designated, and localities and collectors are cited. Biological details are given when available. In all respects the book worthily initiates a series which may be expected to take first rank among those devoted to zoological subjects.

From a postscript at the end it appears that two of the species described by Dr. Aldrich were published a little earlier, under quite other names, by Dr. R. R. Parker. It seems strange that when there are only two persons in the Western Hemisphere working on a subject, they can not consult together sufficiently to avoid such conflicts. Figure 110, as I learn from Dr. Aldrich, though labelled *Sarcophaga bison* is in fact *S. bullata* Parker. The former name was a manuscript one of the author's, and was altered in the text at the last moment, because Dr. Parker published the species as *bullata*.

We hear much these days about the encouragement of research, but it is often overlooked that adequate facilities for publication are essential. Authors are not justified in spending months and years in the preparation of monographs which may never appear in print or serve any useful purpose. There are at this moment many excellent contributions the publication of which is indefinitely postponed, or which must be split up into short papers in order to see the light. To those who are familiar with actual conditions the situation is rather discouraging, and it is not mended by the appearance of a certain number of large books in sumptuous and extravagant form. The Thomas Say Foundation, from necessity no less than choice, publishes as cheaply as is consistent with excellence, and in this respect earns the gratitude of students.

T. D. A. COCKERELL
UNIVERSITY OF COLORADO

THE ORIGIN OF THE PRE-COLUMBIAN CIVILIZATION OF AMERICA

MY attention has just been called to the letters (SCIENCE, October 13, 1916) in which Dr. A. A. Goldenweiser and Mr. Philip Ainsworth Means have put a series of questions for me to answer. As the problems to be solved involve the validity of the foundations upon which has been built up (as the result of more than half a century's intensive studies on the part of leading scholars of every civilized country) a vast superstructure of ethnological doctrine and complex rationalization, perhaps you will afford me the opportunity of replying in some detail to these criticisms, and of adding to the article of mine which appeared in SCIENCE on August 11, 1916, some further reasons for thinking that this elaborate edifice of ethnological speculation will have to be demolished.

While admitting that in the end my contention may be justified, Mr. Means makes the significant comment that "it will be a long time before American anthropologists will be forced to accept these views as final." All that I have attempted to do is to "force" them seriously to examine the foundations of their beliefs, being firmly persuaded that such of them whose minds are still sufficiently alert to be no longer blinded by the outworn dogmas of Bastian and Tylor[1] will be led to accept the views which I have sketched as the only possible interpretation of the facts.

One of the three difficulties suggested by Mr. Means I have already discussed at some length.[2]

[1] By the same mail that brought me the proofs of this letter also came the tidings of the death of the veteran ethnologist whose teaching is so frankly criticized in it. But though his theories of "animism" and "independent evolution of culture" have been a serious factor in clouding the vision of ethnologists, the great merit belongs to Sir Edward Tylor of stimulating a widespread interest in the subject and thereby contributing materially to the advancement of learning, which has earned him the grateful tribute of all scholars.

[2] "Ships as Evidence of the Migrations of Early Culture," *Journal of the Manchester Egyptian and Oriental Society*, 1916.

It is significant that, when citing six memoirs relating to shipping, some of them quite irrelevant, Mr. Means should have omitted all reference to the writings of Pâris, Pitt-Rivers, Assmann and Friederici, where he will find the evidence he imagines to be non-existent. But does the argument from ships really help his case? Where is the "similarity of the working of the human mind" if the highly civilized people of Peru and Mexico hadn't sufficient of what Dr. Goldenweiser calls "happy thoughts" to accomplish more in the way of ship-building? Is not this paucity of shipping merely a token of the remoteness of America from the home of its invention?

The fact that the culture-bearers who first crossed the Pacific by the Polynesian route were searching for pearls and precious metals[3] is surely a sufficient explanation of their desertion of the sea once they reached the American eldorado.

Another of Mr. Means's difficulties I fail to understand. Why was eight centuries too brief a time for a ship to have made its way from the Red Sea to America? Before the introduction of steam-ships what was to prevent a vessel doing the journey as quickly in the eighth century B.C. as in the eighth, or perhaps even the eighteenth, A.D.? There are reasons, given in detail by Aymonier and others, for believing that western culture had already made its influence felt in Cambodia before the close of the seventh century B.C.: Indonesia and even Japan received the leaven at the same time: and it can hardly be in doubt that the ancient mariners did not limit their easterly wanderings to Indonesia, but pushed out into the Pacific, and soon afterwards crossed it to America.

The remaining difficulty which is holding Mr. Means back is that the Pre-Columbian Americans did not use wheeled vehicles. Seeing that the whole of the migration, which I have described as extending from the Red Sea to America, consisted of a series of maritime expeditions, it is not altogether clear what Mr. Means is referring to when he asks:

> Is it not inevitable that they would have made use of such vehicles during their long journey?

At the time the great cultural movement took place it is quite likely that none of the wanderers had ever seen, or even perhaps heard of, a wheeled vehicle. Even if, on some rare occasion of state, in Egypt or one of the Asiatic monarchies, they had seen the king drive in a chariot, was that an adequate reason why these sailors, when, after many years of adventure, they at last reached the American coast, teeming with the spoils they coveted, should have remembered the chariot, and at once set to work to build carts and train llamas to draw them? Surely the utter improbability of this whittles down Mr. Means's difficulty to the vanishing point. Or alternatively, if there is any substance in the "psychic unity" hypothesis, why didn't the Americans get a "happy thought" and invent "so simple and obvious a device" as a wheeled vehicle?

Dr. Goldenweiser's objections are much vaguer and less well-defined. From the latter part of his letter I gather that he is not acquainted with what I have written elsewhere on this subject.[4]

At the outset I must repudiate Dr. Goldenweiser's unwarranted charge that I have "apparently embraced the articles of the Graebnerian faith." My attitude towards the problems of ethnology is that which prevailed

[3] W. J. Perry, "The Relationship between the Geographical Distribution of Megalithic Monuments and Ancient Mines," Manchester Lit. and Phil. Soc. Memoirs, November, 1915; and J. Wilfrid Jackson, "The Geographical Distribution of the use of Pearls and Pearl-shell," *ibid.*, September, 1916.

[4] "On the Significance of the Geographical Distribution of the Practise of Mummification," Mem. and Proc. Manchester Lit. and Phil. Soc., July 7, 1915; republished by the Manchester Univ. Press under the title "The Migrations of Early Culture," August, 1915; "The Influence of Ancient Egyptian Civilization in the East and in America," *Bull. John Rylands Library*, March, 1916; "Ships as Evidence of the Migrations of Early Culture," *Jour. Manc. Egy. and Oriental Society;* and *Nature*, November 25, 1915, p. 340; December 16, 1915, and January 27, 1916, *inter alia.*

amongst most intelligent men until Waitz, Bastian, Tylor, and their innumerable recent disciples, obscured the clear meaning of the facts by a cloud of empty sophistry and misapplied Herbartian philosophy. In many other branches of learning, such as archeology, philology and the history of many of the arts, numerous scholars, who have escaped the vicious influences of this reactionary school, have continued to rely upon facts and interpret their meaning straightforwardly. The writings of Graebner, Frobenius, Ankermann, Foy, Schmidt and Montandon were quite unknown to me when my conclusions were first formulated; their views and mine have nothing in common except that both repudiate the speculations and the antiquated psychology which for far too long have been permitted to hide the truth.

As a guest at the meeting of the British Association in 1911, when Dr. Rivers devoted his presidential address to the discussion of this matter, Dr. Goldenweiser had every opportunity for appreciating the magnitude of the gap that separated his (Rivers's) views from Graebner's. It is straining the truth to brand Rivers as a recruit of the latter's.

The Graebnerian attitude is largely the outcome of the revulsion of modern German opinion against the whole conception of evolution. It included within the scope of its hostility the method in ethnology which has been misnamed "evolutionary."

But the very essence of the conception of evolution is the derivation of all organisms from a common source. It is the teaching of Bastian and Tylor which is a repudiation of evolution; for it is a much closer approximation to the biological idea to look upon similar complex organizations of a series of artificial civilizations as having been derived from the same common source, just as all vertebrate animals were the offspring of one stock, which, after spreading abroad, became more or less specialized in a distinctive way in each locality. To adopt the attitude, which Dr. Goldenweiser is championing, of regarding as the common parent of all these similar customs and beliefs some mystical "psychic unity" is to place ourselves upon the same mental plane as the aboriginal Australian who believes that children are spirits which have entered their mothers in some mysterious fashion.

But, as he devotes the greater part of his criticism to this matter, I must deal with the specific questions he puts to me.

Dr. Goldenweiser asks me to "name *one* ethnologist who can be shown to have attributed similarities in cultures to the working of highly specialized human instincts." Although every ethnologist who subscribes to the modern Tylorian doctrines necessarily adopts a theory of the working of the human mind which, on analysis, can hardly be differentiated from what the modern psychologist regards as instinct—and an instinct which leads men on the two sides of the Pacific independently the one of the other to look upon a serpent equipped with wings and deer's antlers as a power controlling water can hardly be otherwise defined than as "highly specialized"—very few of them, since the time of Daniel Wilson, have had the frankness to admit a fact which would have branded their speculation as a *reductio ad absurdum*. At the meeting of the British Association in 1912 (see Report, p. 607) I discussed this question, and no one attempted to refute the argument that the adoption by two peoples of highly complex and arbitrary practises along with scores of identical and unessential details can be explained only by the assumption of their (*i. e.*, the customs) derivation from a common source, or by postulating human instincts of so complex a kind that no modern psychologist will admit their reality. Several ethnologists accepted the definition of such phenomena as instinctive. Professor Flinders Petrie made the fantastic claim that there was an instinct to build chambered tumuli which could be explained on biological principles. In a written communication Mr. Cecil Firth argued that if the beaver instinctively built his dam, why shouldn't men for analogous reasons build dolmens? But most of my critics stopped short of admitting that such actions were instinctive, though no one attempted to rebut my argument that the modern ethnological

hypothesis when closely analyzed was tantamount to claiming the existence of highly specialized human instincts. I am, of course, not unaware of the way in which this essential question is usually evaded, by the attempt to explain how similar needs, circumstances and environment, can call forth men's activities and shape them so as to lead to identical cultural developments, quite independently one of the other. But such theorizing inevitably ignores the fact that in the majority of cases such identities of culture actually occur under circumstances and to meet needs as dissimilar as they possibly can be. Whereas of two kindred peoples living under precisely similar circumstances in neighboring islands, say in Indonesia or Melanesia, one of them may possess the whole of the complex culture of the stone-using peoples (Perry), and the other not one of the numerous constituent elements of this exotic civilization. It is when we leave vague speculation and consider specific cases that the so-called "evolutionary" doctrine in ethnology collapses.

The common line of argument is that which is displayed in its frankest form by the late Daniel G. Brinton, and his disciples, such as Spinden and Joyce. In his "Myths of the New World," Brinton writes (pp. 126–127):

> No citizen of the United States will be apt to assert that their instinct led the indigenes of our territory astray when they chose with nigh unanimous consent the great American eagle as that fowl beyond all others proper to typify the supreme control and the most admirable qualities,

and he explains what he means by this in the previous paragraph:

> For the winds, the clouds, producing the thunder, and the changes that take place in the ever-shifting panorama of the sky, the rain-bringers, lords of the seasons, and not this only, but the primary type of the soul, the life, the breath of man and the world, these in their rôle in mythology are second to nothing. Therefore as the symbol of these august powers, as messenger of the gods, and as the embodiment of departed spirits, no one will be surprised if they find the bird figure most prominently in the myths of the red race.

This is rationalization pure and simple, which can be proved to be false in every item. For we are now sufficiently acquainted with the earliest literatures of Egypt, Babylonia and India, to know that the association of the eagle or hawk with all these varied phenomena was not due to the reasons Brinton gives. Every one of these manifold attributes became added to the eagle's repertoire as the result of fortuitous circumstances utterly alien to those assumed by Brinton. The mingling of eagle-people with sun-people, and the association of the latter with serpent-people and with the worshippers of Osiris (the controller of water) was the beginning of the complex blending of the symbolism of the sun, the serpent, the eagle and water. In the Babylonian thunder-bird further attributes were added, and others again in India, the Far East and America.

If the followers of Brinton deny that the American thunder-bird came from the Old World they will be faced with this dilemma:—as the origin of the confusion is known (from the earliest Egyptian writings) to be the result of wholly fortuitous circumstances, if the American symbolism (which arrived at essentially the same arbitrary result—on this see Brinton) was developed in a totally different manner, what becomes of the sacred principle of "psychic unity," the "similarity of the working of the human mind"? I wonder which of the two explanations Dr. Goldenweiser would call the "dogmatic or uncritical method"? To indulge in pure speculation, dogmatic assertion and unsupported rationalization, or to go straight to the facts and recognize that the American thunder-bird and the winged snake with deer's antlers certainly came from the Old World?

We can trace the association of the deer with control of the waters from Babylonia along the whole Asiatic littoral, watching the symbolism gradually increase in richness and complexity as, in its passage from west to east, it blends with a variety of other elements, until eventually it emerges in the Chinese dragon, which it supplies with antlers.[5]

[5] I have discussed the whole subject in the forthcoming report of my lecture on "Dragons and Rain-Gods."

In the light of the complex history and the scores of wholly chance circumstances that contributed to the making of this Asiatic wonder-beast, is it at all credible that the Algonkin and Iroquois serpent with wings and deer's horns is an independent invention?

I have so frequently discussed the question of man's inventiveness[6] that it would be unjustifiable to take up more space for this matter here.

When Dr. Goldenweiser claims that Spencer, Tylor, Lubbock, Frazer and Lang "may have neglected to make sufficient use of the concept of the diffusion of culture through historic contact," I agree with him; but I think the words "may have" are superfluous. Yet American scholars, such as Brinton, Hopkins, Spinden and many others, as well as many writers, such as Keane, on this side of the world, have repeatedly attacked Tylor for *over-using* the concept of diffusion.

It is a quaintly piquant situation to find Tylor, who more than any one is responsible for the modern attitude of denial of these cultural migrations, being reproved by his more reckless followers for not pushing his views to the limits of absurdity, and Dr. Goldenweiser, in a letter that is frankly ultra-Tylorian, pretending to hold the scales impartially between the conflicting views.

It is very surprising that so eminent a scholar as Professor Hopkins joins in this attack on Tylor, especially as he can give no reason in justification of his attitude except the flimsy pretext that "we require more proof than Aztec pictures of hell to believe any such theory" ("Religions of India," p. 557, footnote 4). For the very chapter of Hopkins's book where this statement occurs is devoted mainly to the use of precisely the same kind of argument as he condemns when Tylor uses it. He is urging the claim that Indian culture exerted a great influence upon Greece from the sixth century B.C. onwards. The evidence he makes use of is of precisely the same kind as, but infinitely less voluminous and precise than, that which goes to prove an analogous influence of India in America. He rightly claims that "such coincidences are far too numerous to be the result of chance." But if that is so, why is it forbidden to use the same argument in the case of "the pictures of hell"? Are they the sort of thing two peoples would have independently invented?

But Professor Hopkins goes much further than this. In developing the argument (pp. 161 et seq.) that certain elements of culture in India can not be regarded as tokens of Aryan influence, he cites a very remarkable series of exact coincidences between complex Hindu and Iroquois beliefs and ideas. So intent is he upon the demolition of the Aryan argument that he does not seem to realize the more important outcome of his demonstration. For, if it is permissible to use the method of reasoning which he himself employs in the case of Greek borrowing from India, Hopkins has also proved up to the hilt, though without realizing it himself, the Asiatic derivation of many of the religious ideas of the American Iroquois. To quote his own words again, "such coincidences are far too numerous to be the result of chance."

In the light of our present knowledge it is now possible[7] to refer to its original source the germ of a very large number of the elements in the Pre-Columbian civilization of America.

But I should not like Dr. Goldenweiser to mislead the readers of SCIENCE into the belief that I am ignoring considerations of the working of the human mind and of the importance

[6] See for example "Ships as Evidence of the Migrations of Ancient Culture," *Journal of the Manchester Egyptian and Oriental Society*, 1916; also *Man*, February, 1916, p. 27: and if independent witness is desired, see Pitt-Rivers, "Evolution of Culture," p. 91 *et seq.*; the whole question has been discussed by Professor Frederick J. Teggart, of the University of California, in his admirable "Prolegomena to History," 1916, pp. 111 *et seq.*

[7] I have in manuscript an analysis of many scores of American practises, beliefs and myths, each of them traced back to its home in the Old World. Some of these are now being published in the reports of two lectures, "Incense and Libations" and "Dragons and Rain-Gods," in the *Bulletin* of the John Rylands Library.

of local developments in shaping customs and beliefs and giving them their distinctive characteristics. What I object to on the part of ethnologists is not the use of psychological arguments, which are necessarily at the root of the whole matter, but the resort to an effete system of psychology which is utterly repudiated by practically all real psychologists, except Wundt and his disciples.

When a small band of immigrants, intent upon exploiting the mineral wealth, forces its way into a barbarous country, and, in virtue of its superiority of weapons or of skill and knowledge, is able to dominate the local people, and compel it to work for them, the stamp of the alien civilization, its practises, its customs and beliefs, can be imprinted upon a large servile population. Nor must it be assumed that the new learning is adopted wholly and without change. For every people has its own cherished beliefs and customs which no power can wholly eradicate. What happens in such cases is that the new practises are blended with the old; and in course of time, as the mixture becomes more and more intimately rationalized, a new and distinctive cultural compound is developed, which can not strictly be regarded either as the indigenous or the introduced culture, but a new structure which has been built up by the spirit of the local population out of the new and the old materials. Thus even when the same elements of a new culture are introduced into a series of localities the resultant civilizations are not identical; but each takes on its distinctive characters, which are determined partly by the circumstances under which the new leaven has been impressed, and partly by the nature of the pre-existing culture, and possibly to some extent by the character and abilities of the people of the country. For a people's aptitude and inclination to adopt alien practises clearly counts for a good deal in this process. Essentially the same external influences were brought to bear, in varying ways and in different degrees, upon India, Indonesia, Australia, eastern Asia, Oceania and America; but how strikingly different were the results in each of these domains!

The subject, however, is much too vast and intricate profitably to be discussed in a letter. I have already collected enough material for several large volumes on the part played by the "working of the human mind" in the history of civilization. All that I aim at achieving at present is to persuade ethnologists to do what is constantly being done in every *true* science, namely, impartially to examine the foundations upon which its theory rests. If they will consent to do this I have no doubt as to the outcome.[8]

G. Elliot Smith

The University of Manchester, Eng.

THE AMERICAN PHYSIOLOGICAL SOCIETY

The American Physiological Society held its 29th annual meeting in association with the Federation of American Societies for Experimental Biology and the American Association for the Advancement of Science in New York City December 27, 28, 29 and 30. The meetings were all held at the Cornell Medical College buildings where convenient arrangements had been provided also for the other societies of the federation. One of the most pleasant features of the meeting was the arrangement for luncheon, which brought together the members of the different societies.

On December 28 the annual federation banquet was held at the Hotel McAlpin with a large attendance. A similar dinner was held at the Chemist Club December 29.

The new members elected to the society were:

William T. Bovie, Harvard Medical School, Boston.
William John Crozier, Bermuda Biological Station for Research, Agars Island, Bermuda.
Admont H. Clark, Johns Hopkins Medical School.
Frank A. Hartman, University of Toronto.
S. H. Hurwitz, Hooper Foundation, San Francisco.
R. W. Keeton, Northwestern University.
Edward C. Kendall, Mayo Clinic, Rochester, Minn.
Charles E. King, University of North Dakota.
Dean de Witt Lewis, Rush Medical College.
David I. Macht, Johns Hopkins Medical School.
Frank C. Mann, Mayo Clinic, Rochester, Minn.
Victor C. Myers, New York Post-graduate Hospital Medical School.
Oscar H. Plant, University of Pennsylvania.

[8] I think it is only right that your readers should be informed that my article in Science, August 11, 1916, was written in May, 1915, and that by a careless mistake, the uncorrected stenographer's copy was sent to you.

W. C. Quinby, Harvard Medical School.
J. M. Rogoff, Western Reserve Medical School.
C. L. Von Hess, University of Chicago.
Rosaline Wulzen, University of California.

The program of scientific papers and demonstrations was as follows:

IN MEMORIAM

Isaac Ott, A.M., M.D., by Albert P. Brubaker.

Joseph Hoeing Kastle, Ph.D., by A. S. Loevenhart.

Allen M. Cleghorn, M.D., C.M., by Shepherd Ivory Franz.

Thomas Gregor Brodie, M.D., F.R.S., by A. B. Macallum.

"The Fate of Intravenously Injected Dextrose," by J. J. R. Macleod.

"The Diastatic Activity of the Blood in Diabetics," by V. C. Myers and J. A. Killian (by invitation).

"Observations Concerning Fat Feeding," by F. M. Allen (by invitation).

"The Cause of the Increased Heat Production following Pancreatectomy in the Dog," by J. R. Murlin.

"The Use of Cotton Seed as a Food," by Thomas B. Osborne and Lafayette B. Mendel.

"Primary Scrotal Syphilis of the Rabbit and its Utilization in Chemotherapeutic Experiments," by W. H. Brown and L. Pearce (with lantern demonstration).

"The Action of Some Optic Isomers on the Ureter," by D. I. Macht.

"The Influence of Certain Conditions on the Rate at which Epinephrin is Liberated from the Adrenals into the Blood," by G. N. Stewart and J. M. Rogoff (by invitation).

"The Blood Lipoids in Nephritis," by W. R. Bloor.

"Concerning the Lipoids of the Blood in Renal Conditions, with Special Reference to the Cholesterol Content," by Albert A. Epstein and Marcus A. Rothchild (by invitation).

"Observations of Acid Base Equilibrium in the Blood," by John Howland and W. McKim Marriott.

"The Inorganic Composition of Thoracic Duct Lymph," by A. B. Macallum. (Read by title.)

"The Movements of the Artery within the Compression Chamber During Indirect Estimations of the Blood Pressure," by Joseph Erlanger.

"The Time Relations of the Fundamental Heart Sounds," by Carl J. Wiggers and A. Dean, Jr. (by invitation).

"The Veno-pressor Mechanism," by Yandell Henderson.

"Further Observations on the Distribution of Blood in Shock," by H. C. Jackson and H. H. Janeway (by invitation). (Read by title.)

"The Effect of Pneumothorax in the Dog and Cat," by H. C. Jackson and C. J. Imperatori (by invitation).

"A Comparison of the Effects of Food and of Caffeine on Work in the Athlete and an Untrained Subject," by C. B. Root (by invitation) and H. Curl (by invitation). (Read by title.)

"Effects of Physical Training on Pulse and Blood Pressures During Activity and During Rest," by Percy M. Dawson.

"Some Electrical Phenomena of Animal Tissues," by Robert A. Gesell.

"Experiments on the Relation of Blood Pressure to Urine Formation," by A. N. Richards and O. H. Plant (by invitation).

"The Fate of Sulphophenolphthalein when Injected into the Animal Organism: Factors other than the Kidney Influencing its Retention," by E. C. Kendall (by invitation).

"Action of the Diuretics on the Denervated Kidney," by Wm. C. Quinby (by invitation).

"On the Movements of the Isolated Ureter of the Dog," by George B. Roth.

"The Stimulating Action of the Bromide Ion on Smooth Muscle," by T. K. Kruse (by invitation).

"The Physiology of the Chromatophores of Fishes—II. Responses to Alkaline Earths and to Certain Neutral Combinations of Electrolytes," by R. A. Spaeth.

"The Absorption of Fat in Depancreatized Dogs," by Joseph H. Pratt, C. W. McClure (by invitation) and Beth Vincent (by invitation).

"On the Toxemia of Intestinal Obstruction," by L. R. Dragstedt (by invitation), W. Burby (by invitation) and A. J. Carlson.

"The Visible Structure of Cell Protoplasm, and Death Changes," by Robert Chambers, Jr. (by invitation).

"The Effect of a-Amino Acids, Dipeptids and Peptones on the Growth of Cells in Vitro," by Clarence A. Neyman (by invitation) and Montrose E. Burrows (by invitation).

"Muscular Irritability," by C. C. Guthrie.

"Studies in Muscular Power and Fatigue," by A. H. Ryan and J. H. Agnew (by invitation).

"A Comparison of the Amount of Catalase in the Muscles of Active and Inactive Animals," by W. E. Burge. (Read by title.)

"A Study of the Total and Preformed Creatinin in Various Muscles of the Cat," by Ernest L. Scott and Adelaide Spohn (by invitation).

"The Cardio-skeletal Quotient," by W. L. Mendenhall.

"Hunger and Appetite in Fever," by A. J. Carlson, J. Mayer (by invitation) and J. R. Rupp (by invitation).

"The Influence of Temperature on the Gastric Hunger Contractions of Some of the Lower Animals," by T. L. Patterson.

"The Mechanism of the Regulation of the Intra-abdominal Pressure," by Helen C. Coombs (by invitation).

"Preliminary Report of Cardiogram and Blood Pressure Records, showing the Effect of Music," by Ida Henrietta Hyde. (Read by title.)

"Further Observations on the Existence of a Cerebral Heat Center," by Ernest Sachs and P. P. Green (by invitation).

"An Application of Boyle's Law and Avogadro's Hypothesis to the Oscillations of the Mercury Manometer," by Albert M. Bleile (by invitation).

"The Action of an Oxidizing Substance on the Catalase Value of the Blood," by Aaron Arkin.

"The Hydrolytic Products of Chitin," by S. Morgulis. (Read by title.)

"Water Vaporization in Health and Disease," by Eugene F. Du Bois and G. F. Soderstrom (by invitation).

"Some Considerations of the Isolated Bone Marrow," by Cecil K. Drinker, Henry A. Kreutzman (by invitation), and John R. Paul (by invitation).

"Some Physiological Disturbances Induced in Animals by Nitrobenzol Fumigation," by Melvin Dresbach and W. L. Chandler (by invitation).

"Physiological Effects of Ingestion of Ethyl Alcohol by Rectum, with Special Reference to the Gaseous Exchange," by T. M. Carpenter.

"On the Reflex Control of the Vagus Tonus," by F. T. Rogers (by invitation).

"An Instance of Apparent Anesthesia of a Solution," by E. N. Harvey.

"Labyrinthine Reactions in Kittens, with Demonstration," by A. L. Prince.

"Minimal Variations in Spinal Reflex Thresholds," by Eugene L. Porter.

"Possible Periodic Variations in the Knee Jerk in Women," by Jessie L. King.

"The Combination of Thrombin by the Antithrombin of the Blood Serum," by H. S. Gasser.

"The Transformation of the Plasma Clot," by George A. Baitsell. (Read by title.)

"Inhibitory Effects of Adrenalin upon the Sphincter of the Iris," by Don R. Joseph.

"Changes in the Percentage Composition of Muscle Protoplasm during Prolonged Fasting with Work," by C. W. Greene.

"Some Observations on the Conditions of Activity in the Adrenal Glands," by W. B. Cannon and H. F. Pierce (by invitation).

"Further Studies on the Effects of Adrenalin on Muscular Fatigue and Limb Circulation," by Chas. M. Gruber.

"Anti-Neuritic Substances from Egg Yolk," by H. Steenbock.

"Endermic Reactions," by J. D. Pilcher and T. Sollman.

"The Active Constituent of the Thyroid; Its Isolation, Chemical Properties, and Physiological Activity," by E. C. Kendall (with lantern demonstration).

"On the Mechanism of Blood Coagulation," by G. H. A. Clowes.

"The Perfusion of the Mammalian Medulla: The Effect of Carbon Dioxide and other Substances on the Respiratory and Cardiovascular Centers," by D. R. Hooker and D. W. Wilson.

"Recovery Experiments Following Removal of the His Auricular Node in the Dog's Heart," by J. A. E. Eyster and W. J. Meek.

"The Effects of Temperature Change on Rhythm in the Human Electromyogram," by Alexander Forbes and W. C. Rapleye (by invitation).

"The So-called Experimental Streptococcic Poliomyelitis," by C. G. Bull (by invitation) (with lantern demonstration).

"Experimental Studies in Poliomyelitis," by E. C. Rosenow and G. W. Wheeler (by invitation).

"Experimental Poliomyelitis in the Monkey," by E. C. Rosenow and E. B. Towne (by invitation).

"The Relation of the Velocity of the Pulse Wave to Blood Pressure," by Percy M. Dawson. (Read by title.)

"Does the Myelin Sheath Function as an Insulator Broken only at the Nodes of Ranvier?" by J. F. McClendon. (Read by title.)

"The Oxygen Pressure Necessary for Cellular Activity," by Montrose T. Burrows (by invitation). (Read by title.)

"The Cause of Cataract," by W. E. Burge. (Read by title.)

"The Attenuation of the Toxic Action of KSCN by Elevated Temperature," by R. A. Spaeth. (Read by title.)

"A Signal Magnet which Writes Either Upwards or Downwards," by W. Hale.

"Some New Apparatus," by D. E. Jackson.

"An Improved Lever for Frog's Heart and Muscle Strips," by A. H. Ryan.

"The Inhibitory Effect of Stimulation of the Central End of the Vagus Nerve upon the Contractions of an Active Expiratory Muscle in the Chicken," by A. L. Meyer (by invitation).

"Demonstration of a Gas-Analysis Apparatus," by Yandell Henderson.

"The Motion Picture as an Aid in Teaching Physiology," by J. A. E. Eyster and W. J. Meek.

"Pathescope Films used to Illustrate Physiological Demonstrations to Students," by Alexander Forbes.

"Motor Phenomena of the Stomach and Cap as Observed Roentgenographically," by Gregory Cole (by invitation).

"Photographs Representing the Growth of Chickens Fed with Definite Mixtures of Foodstuffs under Laboratory Conditions which have heretofore not Led to Success," by Thomas B. Osborne and Lafayette B. Mendel.

"A Convenient Form of Non-Polarizable Electrode for Class Use," by Theodore Hough.

"Microscopic Demonstration of Absence of Chromatolytic Change in the Central Nervous System of the Woodchuck (*Marmota monax*)," by A. T. Rasmussen (by invitation) and J. A. Myers (by invitation).

"Glycogen in the Blood Vessels of the Liver," by G. Carl Huber and J. J. R. Macleod.

"A Method of Recording Fundamental Heart Sounds Directly from the Heart," by Carl J. Wiggers and A. Dean, Jr. (by invitation).

"Exhibit of Photographically Recording Apparatus for Studying the Dynamics of the Circulation," by Carl J. Wiggers.

The final joint meeting of Saturday afternoon proved to be of unusual interest, especially as regards a group of papers on the subject of poliomyelitis, or infantile paralysis. This discussion brought out the largest attendance of the entire scientific series of meetings.

The annual session adjourned with very kindly feelings of the membership for the local committee and the staff of the Cornell Medical College for the convenient and genial arrangements providing for the physical comforts of the session.

Chas. W. Greene,
Secretary

Columbia, Mo.